

The observations show conclusively that we have here one of the most rapid binary systems known, the angular motion in the last two years being not less than 80° . It is too early to speculate upon the probable period, but the measures of the next three or four years will go far to give at least an approximate determination. It is to be hoped that double-star observers will give this interesting system special attention. By next year it will probably be comparatively easy of measurement. An increase in the distance of one-tenth of a second will be sufficient for this.

Chicago,
1880, December 15.

Remarks on Dr. Hartwig's Recent Determination of the Lunar Physical Libration. By the Rev. C. Pritchard, Savilian Professor of Astronomy at Oxford.

It will be in the recollection of the Society, that in June last I announced the completion of the mathematical and computational parts of the investigation of the Lunar Physical Libration, as derived from 59 measures of Lunar Photographs, and that another similar set, made with reference to a second spot upon the Moon, was nearly finished. Meanwhile Dr. Hartwig has just published a memoir detailing the results of 42 measures, made at Strassburg with a Heliometer, for the attainment of the same object. That memoir, I do not doubt, deserves and will find an honourable place in the Annals of Astronomy, and he will himself meet with the sympathy of astronomers on the successful termination of his labours in this behalf.

I find that the results obtained by Dr. Hartwig are in substantial, though not absolute, agreement with my own. An absolute agreement was scarcely to be expected, owing, not to the difficulty and to the length of the various processes employed, but to the fact that the principal quantity ultimately sought for could not considerably exceed a second of arc when seen from the Earth; and, independently of this fact, any inevitable errors in certain parts of the processes must entail 250 times the same amount of error in the ultimate result.

I will now so far anticipate the communication which I hope speedily to lay before the Society, as to place in juxtaposition some of the principal results of the researches in which we, and a few other astronomers, have been engaged.

The first of these researches was that published by Bouvard, at Paris, in 1822; the second by Kreil, at Milan, in 1837; the third by Wichmann with the Königsberg Heliometer in 1848; the fourth by Hartwig, with a Heliometer at Strassburg in 1880; and, finally, that by myself at Oxford, with photographs taken

in the focus of the De la Rue Reflecting Telescope, of which notice was given in the summer of 1880.

First, as to the mean Inclination (I) of the Lunar Equator to the Ecliptic; secondly, as to the maximum amount of the Lunar Physical Libration in Selenographic Longitude (L).

	I.	L.
Paris Determination	° 1 28 45	289° 7
Milan	1 35 48	342° 6
Königsberg	1 32 9	389°
Strassburg	1 36 34	207°
Oxford	1 32 58	350°

With regard to the last three, I do not think that, taking all the elements of the case into consideration, there exists ground for expecting a closer agreement, the reasons for which opinion will be stated in the sequel.

It is now necessary to state that the entire foundation of the researches hitherto undertaken, has been the measurement of the distances of some well-defined spot on the Moon, from various points in its periphery; and, in general, seven or eight such measures have been taken for a single observation. Wichmann and Dr. Hartwig selected the same spot, Mösting A. At Oxford, two spots have been selected, one of which, Triesnecker B, has the advantage of being much nearer to the Moon's mean centre than is Mösting A. In the course of the investigation the Selenographical Longitudes and Latitudes of these spots become necessarily determined. The results obtained by Wichmann and Dr. Hartwig are not in absolute accordance. They are as follows :—

	Wichmann.	Hartwig.
Selenographic Longitude of Mösting A	° 5 13 23	5 10 57
Selenographic Latitude	3 10 55	3 10 21

These coordinates imply a discordance of nearly three minutes of a great circle, as seen from the Moon's centre, but as seen from the Earth they indicate an uncertainty of a little under a second of arc. It may here be proper to observe that notwithstanding these different determinations of the coordinates of the same spot by two highly accomplished and successful observers, each employing upwards of forty distinct determinations, Dr. Hartwig comes to the conclusion that the probable error of a single determination of place, as seen from the Earth, is only $\pm 0''\cdot43$. I think it becomes a question whether the discordance may possibly and properly be assigned to the use of instruments having different apertures. The fact exists. Does this also tend to explain the discordances in the determination of the inclination of the Moon's Equator to the Ecliptic?

It is also desirable to remark on Dr. Hartwig's method of avoiding errors arising from any irregularities in the contour of the Moon's limb. He takes seven places on the limb, distributed with considerable uniformity; but, in making a measure of the distance of the spot with the Heliometer, wherever his eye could perceive the necessity for it, he has regard to what appears in his judgment to be the average level of the contour.* I have myself adopted a different method, which is this: inasmuch as the photograph lies quietly and undisturbed under the microscope of the De la Rue Micrometer, an opportunity is given of leisurely selecting those places where little or no serrature exists. I may add also that, in the case of photography, there are no changes of the Moon's diameter during the time of observation. Changes of this nature necessarily introduce considerable trouble in the heliometer determinations.

While on this subject of the irregularities of the Moon's limb it is proper to refer to what Dr. Hartwig states as the result of his own experience. He altogether and decidedly declines to accept what he terms the emphatically expressed conviction of Mr. Neison as regards the introduction of serious periodical errors from this source. My own long experience is in entire accordance with the opinions of Dr. Hartwig.†

I may also add that after listening to Mr. Neison's strictures on this behalf, I instituted a series of measures round the entire visible periphery of a Lunar photograph, avoiding no irregularities whatever; and the result was such as convinced me that the irregularities themselves, in this particular case, did not interfere with the exactness of computed results. But there was another conclusion which was unavoidably impressed on my own mind, from these and similar researches. Dr. Hartwig, in the course of his admirable Memoir, refers to the uncertainty of the determination of the Selenocentric Longitudes when compared with those of the Selenocentric Latitudes. This comparative insecurity which has been pointed out by Wichmann, had been sensibly observed by myself. Experiments at last indicated its probable cause; and, towards the conclusion of the work, I discovered that the source of error might be very much avoided by increasing the number of measures, and by distributing them in particular and ascertainable directions. The true cause originates, I believe, in the necessary want of symmetry of the Moon's illuminated periphery, with respect to the selected spot on the Moon's disk. The configuration of the spot, relative to the illuminated periphery, is always unavoidably one-sided in Longitude. Owing to this cause, Dr. Hartwig, and Wichmann before him, considered themselves justified in applying certain weights (more or less arbitrarily determined) to the Latitude equations of condition. I have myself hesitated, and am still

* Cf. p. 27, Dr. Hartwig's *Memoir*.

† Page 31, *Op. cit.*